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gr., while, when voluntarily lifted, 1000 gr. and 1500 gr. were distinguished. In the case of faradic stimulation, practically no discrimination of weights was possible. This removes one of the strongest experimental arguments for the exclusively peripheral estimation of weights.

Ingenious as Waller's main argument is, it leaves two loop-holes of escape from his conclusion: (1) Granted that in fatigue the centre is mainly exhausted, it does not follow of necessity that this centre is the point of "incidence" of the feeling of fatigue. Mosso shows that central exhaustion has a chemical peripheral effect; this may occasion the feeling of fatigue. Or central expenditure may draw a kinæsthetic centre apart from itself, this latter contributing the feeling of fatigue. Waller's inference is a highly probable one, but not the only possible one. Further granted for the moment that the inference is correct, it only puts the sense of fatigue somewhere in the centre, not necessarily in the motor seat, unless we agree with Waller in denying the distinction between sensor and motor rests. (2) Granting the full value of the experiments, they again go no farther than to render probable the point of "incidence" of the sense of effort. Such a feeling may still arise in a kinæsthetic centre in dynamic connection with the working motor centre, or from "remote" peripheral courses. Nevertheless, logical alternatives aside, Waller's conclusion is undoubtedly the best interpretation of Waller's premises.

DELABARRE, *The Influence of Muscular States on Consciousness; Mind.* N. S. 3, July, '92.

This paper is largely a summary of the author's *Ueber Bewegungsempfindungen*, which will be noticed later. In this connection, only Delabarre's criticism of Waller may be referred to. Delabarre claims that Waller's argument contains three assumptions: (1) "That the objective signs of exhaustion are always indicative of a previous expenditure of energy in the same parts." [Not so. Waller uses the same muscle for voluntary and direct excitation. His assumption is: Granted both voluntary and direct work from the same muscle and objective exhaustion only for voluntary work, then the objective exhaustion must be in the nervous centre for this muscle. This is valid, and Delabarre's statement is a fallacy of conversion.] (2) "That a subjective sense of fatigue is indicative of a corresponding previous effort, and sense of effort, in the same parts." [Not so. Another involved conversion. Waller's position is: Granted a maximum voluntary effort and sense of effort directed to a part, and a sense of fatigue following the exercise of the same part, then the seat of this sense of exhaustion is the same as the seat of the sense of effort. This does not necessarily follow, as I have said above, but is physiologically extremely probable.] (3) "That objective signs of exhaustion are indicative of a subjective sense of fatigue, and objective signs of effort of a subjective sense of effort, localized in the same parts." [I also find this assumption, and have above expressed an opinion of the extent to which it invalidates Waller's conclusion.]

MÜNSTERBERG, *Mitbewegungen, in Beiträge zur experimentellen Psychologie*, Heft IV., 192.

Professor Münsterberg gives a résumé of the discussion on the question of the concurrent innervation of symmetrical movements on the two sides of the body, against the old view that there was a direct tendency to such symmetrical movements when either side

is stimulated; he opposes both theoretical and experimental considerations. His experiments, executed in co-operation with several of his students, consisted in the voluntary performance of preconceived drawing and tracing movements (circles, triangles, squares, etc.), with one or both hands; then in occupying the attention with one hand only and observing the behavior of the other hand; or in withdrawing the attention altogether and observing the behavior of both hands, etc., through the various combinations available. His results lead him to the conclusion that in movements up and down and away from the body, such symmetrical movements do not occur; but that in movements right and left from the body such movements do occur. These latter cases he explains as due to the maintaining of the equilibrium of the body. His conclusion is that "inborn symmetrical co-ordinations of the muscles of the extremities do not exist." [This conclusion is by no means proved by M.'s interesting experiments. In the first place, the present writer finds it almost impossible to keep the attention so constant as not to interfere with the so-called "Mitbewegungen." Further, if Waller's result be true, there would be a certain residual discharge in the muscle after voluntary attention is withdrawn from it and this ought to give a certain amount of movement, symmetrical or asymmetrical, according to the voluntary movement. It is quite possible that there is such an element, but that it is drowned in the grosser tensions due to equilibrium, maintenance of balance, required habit, etc. Such a tendency could only be measured by a graphic record under conditions which ruled out the grosser sources of error; not by the rough explanation of a group of people standing or seated around a table. But, more than this, is experiment on adults likely to throw any light on this question at all? Everybody admits that our adult movements are massed in asymmetrical co-ordinations, which represent the strongest dynamic tendencies. There are, also, facts on the affirmative side of the question, such as "crossed reflexes" (see this Journal V. '92, p. 84). In the case of infants we have "crossed" responses in sleep (see my observations in *Science* XIX., 1892, p. 16, and Preyer's *Mind of the Child*, I., p. 207 ff). M.'s citation (p. 195) of my observations in connection with the development of right-handedness (resumed above) does not take account of the fact that in reporting these observations I added, a little later on, "In many cases the left hand followed slowly upon the lead of the right;" this was also true in the cases in which the left hand led—the right followed after in the same direction. A crucial test of the general question might be reached by Gotch and Horsley's new electrical method; the galvanometer showing to what extent, if at all, the cortical stimulation of a muscular group affects the motor nerves of the opposite side. As far as Gotch and Horsley's results on cats and monkeys bear on the question, they indicate that the tendency to bilateral performances is relative to the intensity of the stimulation—what we would expect from the general principle of diffusion. (Gotch and Horsley, "On the Mammalian Nervous System, etc.," Croonian Lecture, *Phil. Trans.*, 1891.)]

SCHENCK, *Über den Erschlaffungs Process des Muskels* Pflüger's *Archiv*, LII., 1892, p. 117.

S. asks the question: Why is it that the down-slope of the curve of contraction of a muscle exhausted by cooling is as steep as its up-slope, while the down-slope of that of a voluntarily exhausted muscle is not as steep as its up-slopes? He surmises that the less